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Part One
GENERAL PAPERS

MITCHELL ON WHAT HAPPENS DURING BUSINESS CYCLES*

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Shortly before his death Wesley Mitchell put in my care the completed parts of the "progress report" he was preparing on his long and elaborate investigation of "what happens during business cycles". This book is substantially the document he left behind. I have felt free to make numerous changes of detail, but I have not interfered with the design, nor attempted to complete the narrative. The work of a major scientist, even if not half done, deserves a life of its own, unencumbered by the hand or voice of another. So it is especially when, as in the present case, the fragment has well defined contours, balance, and direction. But for the guidance of students who may take up the book for the first time, I shall put down a few remarks about Mitchell's objectives and what he accomplished.

I

Business cycles are not merely fluctuations in aggregate economic activity. The critical feature that distinguishes them from the commercial convulsions of earlier centuries or from the seasonal and other short-term variations of our own age is that the fluctuations are widely diffused over the economy – its industry, its commercial dealings, and its tangles of finance. The economy of the western world is a system of closely interrelated parts. He who would understand business cycles must master the workings of an economic system organized largely in a network of free enterprises searching for profit. The problem of how business cycles come about is therefore inseparable from the problem of how a capitalist economy functions.

This conception governs Mitchell's posthumous book, as it does his

* The original plan of the Conference called for a paper by Wesley Mitchell. Although this part of the plan had to be dropped on account of Mitchell's death, it was nevertheless felt that the Conference should give some attention to his work. At the request of the Planning Committee, Arthur F. Burns distributed copies of a manuscript on business cycles that Mitchell had been working on for some years. This study, *What Happens during Business Cycles: A Progress Report,* was published in 1951 by the National Bureau as Number 5 of its Studies in Business Cycles. The paper here printed is Mr. Burns' Introduction to that volume. It is a revision and extension of his remarks at the Conference.

earlier writings. Mitchell was not content to focus analysis on the fluctuations of one or two great variables, such as production or employment. His concern was with *business* cycles and he therefore sought to interpret the system of business as a whole – the formation of firms and their disappearance, prices as well as output, the employment of labor and other resources, the flow of incomes to and from the public, costs and profits, savings and investments, the merchandising of securities as well as commodities, the money supply, its turnover, and the fiscal operations of government. Not only that, but he sought to penetrate the facade of business aggregates and trace the detailed processes – psychological, institutional, and technological – by which they are fashioned and linked together.

Thus Mitchell took as his scientific province a terrain as far-flung and intricate as Walras' and Marshall's. But he explored more fully than his predecessors the obstacles to the mutual adjustment of economic quantities in a disturbed environment. "Time . . . is the centre of the chief difficulty of almost every economic problem."[1] Pursuing this Marshallian theme through uncharted jungles of statistics, Mitchell detected systematic differences in the rates of movement of economic variables, and arrived at an early stage of his scientific work at the conception that our economic system of interdependent parts generates a cyclical path instead of moving toward an equilibrium position. This fateful twist aside, Mitchell's economic outlook was thoroughly Marshallian. Had he lived to finish this book, he would have inscribed on its title page Marshall's motto: "The many in the one, the one in the many."

The hypothesis that each stage of the business situation tends to develop out of the preceding stage and to grow into the next in a cyclical pattern poses two major questions: Does economic life actually proceed in recurrent fluctuations having similar characteristics? If so, by what processes are continuous and repetitive movements of this character brought about? In a search for definite and dependable answers, Mitchell examined "facts on a wholesale scale", as had Darwin before him in a related field, and Lyell before Darwin. "My success as a man of science", wrote Darwin, "has been determined . . . by complex and diversified mental qualities and conditions. Of these, the most important have been – the love of science – unbounded patience in long reflecting over any subject – industry in observing and collecting facts – and a fair share of invention as well as of common sense."[2] These, too, were the sources of Mitchell's scientific strength. In his quarto on *Business Cycles,* published in 1913, he anchored a theory of fluctuations to an array of empirical observations unprece-

[1] Alfred Marshall, Preface to the first edition of his *Principles.*

[2] Charles Darwin, "Autobiography", in *Life and Letters,* edited by Francis Darwin (D. Appleton & Co., 1888), Vol. I, pp. 68, 85-6.

dentedly full for its time. But Mitchell was not content with this achievement. World War I had ushered in a new era of economic statistics, able theorists were elaborating new hypotheses, and statistical analysts were rapidly fashioning new devices for disentangling economic movements. Eager to exploit the new materials for research, Mitchell launched in 1922 a fresh investigation of business cycles.

II

The science of economic fluctuations is only beginning to pass into an inductive stage. Even today the descriptions of business cycles by economists often resemble the descriptions of plant life by writers of antiquity, who commonly relied on "casual observations, no experiments and much speculative thinking".[3] If later botanists often "could not identify the plants by the descriptions", so it has also been in economics. As long as investigators worked by themselves, they could not very well "collect the masses of raw data pertinent to the study of cyclical behavior, segregate the cyclical components from movements of other sorts, and assemble the findings to form a realistic model of business cycles by which explanations could be judged".[4] In recent decades the organization of scientific institutes has greatly enlarged the possibilities of empirical research in economics. Mitchell made the most of the opportunity afforded by the resources of the National Bureau. Taking his own and others' explanations of business cycles as "guides to research, not objects of research" (p. 5), he delved deeply into the facts of cyclical behavior and the relationships among them. The wish to contribute to economic policy was strong in Mitchell. Stronger still was his conviction that intelligent control of business cycles depends upon sound theoretical understanding, which requires tolerably full and accurate knowledge of what the business cycles of experience have been like.

Business Cycles: The Problem and Its Setting, the first major instalment of Mitchell's investigation, was published in 1927. The second appeared in 1946 under the title *Measuring Business Cycles.* In the meantime numerous studies of special aspects of cyclical fluctuations were prepared by the Bureau's staff, and a small group was steadily engaged in analyzing the cyclical behavior of economic processes.[5] It was Mitchell's hope to integrate the findings of his collaborators with his own and other investigators' results; that is, to develop a model of business cycles from carefully

[3] William Crocker, "Botany of the Future", *Science,* Oct. 28, 1938, pp. 387, 388.

[4] Mitchell, *What Happens during Business Cycles,* p. 4. All other page references unless otherwise indicated, are to that volume.

[5] See the list of publications on business cycles at the end of this volume.

screened observations, to use it in explaining how the cycles of experience are typically propagated, and then press on to account for the outstanding differences among them.[6] But he would have fallen short of the goal even if he had lived to complete the present book. Many of the needed materials – especially for foreign countries – were not in shape for use, and the subject of business-cycle differences required systematic investigations yet to be undertaken. As it stands, Mitchell's report barely covers the first three of the seven parts he had planned. Part I sets out his aims, methods, and materials. Part II deals with the great variety of cyclical movements characteristic of individual economic activities. Part III, not fully completed, shows how the cyclical movements of different parts of the American economy fit together into business cycles, and paves the way for analyzing the processes of expansion, recession, contraction, and revival, to which the last four parts were to be devoted.

Thus the book is a 'progress report', both in the sense in which Mitchell intended the phrase and in the poignant sense forced by his death. Yet no existing publication elucidates so fully or so authoritatively what happens during business cycles as Mitchell's fragment. The accent of the book is on characteristic behavior, formalized in the concept of a 'typical cycle'. "The only normal condition" of business, as Mitchell once expressed it, "is a state of change";[7] but some states of change are 'normal' and others 'abnormal', and Mitchell's 'typical cycle' is designed to take account of such differences. Hence, this concept is similar in some respects to the classicists' 'normal'. The role of each is to segregate the effects of complex causes: both are devices of abstraction: both are tools for analyzing new, concrete situations. Mitchell was keenly concerned about the wide variations among the business cycles of experience and eager to press investigations of them. But he deemed it essential, as a first step, to lay bare the typical characteristics of the alternating waves of prosperity and depression that have swept the economic world in modern times. In this emphasis he conformed to the usual practice of business-cycle theorists. He broke with tradition, however, by extracting what is 'typical' or 'aberrant' from mass observations, and thus substituting fact and measure, as well as may be, for the impressionistic judgments that have ruled business-cycle literature.

III

Mitchell begins his survey of what happens during business cycles by illustrating the varieties of behavior characteristic of economic activities

[6] For a fuller account, see "Wesley Mitchell and the National Bureau", in the Bureau's *Twenty-Ninth Annual Report*.

[7] *Business Cycles: The Problem and Its Setting*, p. 376.

in the United States. Some of the figures in his introductory chart merely confirm common knowledge. For example, commodity prices generally rise and fall with the tides in production; business failures increase during contractions of aggregate economic activity and diminish during expansions; the output of durables fluctuates more widely than the output of perishables; and prices are more stable at retail than at wholesale. It is less generally known, however, that crop production moves rather independently of business cycles, or that production typically fluctuates over a much wider range than prices, that the liabilities of business failures usually turn down months before economic recovery becomes general and turn up months before recession, that both durables and perishables experience their most vigorous decline well before the end of contraction, and that retail prices characteristically move later as well as less than wholesale prices.

Students who will take the trouble to ponder these facts are not likely to leave Mitchell's chart quickly. They will notice that orders for investment goods tend to lead the tides in aggregate activity, that private construction is more closely related to business cycles than public construction, that call money rates or even commercial paper rates greatly overstate the fluctuations in the rates of interest at which bank customers ordinarily borrow, that interest rates in New York tend to move before and more widely than in the interior, that the number of business failures lags behind the liabilities, that bond prices tend to lead stock prices which themselves lead the turns in aggregate activity, that bank deposits appear to be comparatively steady during depressions, that imports conform closely to business cycles while exports do not, that grocery sales fail to show the regular response to business cycles characteristic of retail trade at large, etc. And if the reader looks beyond the large processes that have dominated theoretical literature, he will see how peculiar the cyclical behavior of smaller sectors of activity can be. For example, cattle slaughter tends to move with the tides in aggregate activity while hog slaughter moves inversely; the dollar volume of residential construction contracts fluctuates less, not more, than the physical volume; cotton stocks held at mills run parallel with mill production, while stocks in public storage move inversely.

Thus business cycles are complex phenomena – far more so than has been commonly supposed. The sales of a large firm may be dominated by the tides in aggregate activity; the fortunes of a small firm are rather at the mercy of personal factors and conditions peculiar to the trade or locality. Some activities, like local transit or net gold movements between the United States and Great Britain, are apparently free from cyclical fluctuations. Others, notably farming, undergo cyclical movements, but they have

little or no relation in time to business cycles. And these irregular responses, passed over lightly by theoretical writers, accord with reason:

> We cannot expect any activity to respond regularly to business cycles unless it is subject to man's control within the periods occupied by cyclical phases, and unless this control is swayed, consciously or not, by short-period economic considerations. The domination of harvests by weather, the 'migratory property' of petroleum underground, the mixed motives of governments in undertaking construction work, the long-range planning that weighs with many men in a position to set 'administered prices', the time-consuming negotiations that prevent prompt adjustments of certain other prices and many wage rates, the existence of long-term contracts, the years required to complete some large undertakings – these are concrete examples of the multifarious obstacles that interfere with prompt and regular response to the cyclical tides (p. 95).

The processes that fail to bear the imprint of business cycles are nevertheless a minority. Almost nine-tenths of Mitchell's basic sample of approximately 800 time series fluctuate in sympathy with the tides in aggregate activity, but the movements of this imposing majority are far from uniform. Between the cyclical recalcitrants like farming and the cyclical regulars like factory employment, there is a continuous gradation. Coal and iron production conform more closely to the tides in aggregate activity than the production of textiles or gasoline. The prices of industrial commodities do not conform as well as their production, while the opposite relation rules in farming. Employment conforms better than wage rates, bank loans than investments, open-market interest rates than customer rates, stock prices than bond prices, etc. Some conforming processes move early in the cyclical procession; for example, orders for investment goods. Others, like interest rates, are laggards.

Of course, most processes respond to the tides in aggregate activity by rising during expansions and declining during contractions, though they may do so with a lead or lag. But business cycles also generate countercyclical movements:

> Brisk business increases the domestic demand for textile goods and so diminishes the exports of raw cotton; it increases the sale of fresh milk and so restricts the production of butter; it increases the volume of coin and paper money held by the public and stimulates borrowing from the banks, thereby enlarging demand liabilities and tending to impair reserve ratios; it leads department stores to carry larger stocks of merchandise and lowers the piles of iron ore at blast furnaces; it activates share transactions on stock exchanges and discourages transactions in bonds. The declines in this list, and many others, are as characteristic a feature of business cycles as the advances (p. 66).

However, the processes that run counter to business cycles do so, by and large, with less regularity than those that respond positively. An expansion of money incomes stimulates a general increase in buying, and this

influence may obscure the concomitant impulse to shift demand away from inferior articles to goods of higher quality. As it turns out, purchases of staples such as pork, flour, coffee, and potatoes frequently decline during expansion, but their inverted response is less regular than the positive response of more costly articles.

> In general, influences that tend to repress an activity in expansion encounter more opposition than influences favoring an increase, and when repressing influences win out, their victories are less regular from cycle to cycle than the victories won by influences that push forward. *Mutatis mutandis,* the like holds true in contraction (p. 96).

Large as are the variations in the cyclical timing of economic processes, the differences in amplitude of fluctuation are more impressive still. In high grade bond yields, for example, the cyclical wanderings are confined to a narrow range; the total rise and fall is typically only about 10 per cent of their average value during a business cycle. The amplitude of the overall index of wholesale prices, excluding war episodes, is nearly twice as large; the amplitude of factory employment four or five times as large, of private construction contracts over ten times and of machine tool orders over twenty times as large. On the other hand, stocks of industrial equipment are remarkably steady, expanding usually during contractions as well as expansions of business cycles. The proportions among economic quantities keep changing so systematically over a business cycle that the

> very essence of the phenomenon is omitted unless the chart of business cycles contains numerous lines that indicate the wide differences among the rates at which, and also some of the differences in the times at which, various elements in the economy expand and contract. For, unless these divergencies in cyclical behavior are pictured by fit symbols, we have no suggestion of the basic business-cycle problem: how an economic system of interrelated parts develops internal stresses during expansions, stresses that bring on recessions, and how the uneven contractions of its varied parts pave the way for revivals (p. 295).

IV

So much for the varieties of cyclical behavior that come to the surface once the lid is lifted from aggregate activity. What sort of whole do the parts make up? When the individual pieces are put together it appears that every month some activities reach cyclical peaks and others decline to their troughs; so that expansion and contraction run side by side all the time. But the peaks tend to come in bunches and likewise the troughs. Hence, when troughs gain on the peaks, expansions grow more numerous and in time dominate the economy. Their supremacy is short lived, however, and gradually gives way to the encroachments of contraction. The business cycle of experience is the alternating succession of these sustained

majorities – first of individual expansions, next of contractions, then of expansions once again, and so on.

> Business cycles consist not only of roughly synchronous expansions in many activities, followed by roughly synchronous contractions in a slightly smaller number; they consist also of numerous contractions while expansion is dominant, and numerous expansions while contraction is dominant (p. 79).

According as the expansions or contractions of individual activities dominate, the aggregate activity of the economy surges forward or recedes. And when economic crosscurrents are at or near their maximum, the direction of aggregate activity is reversed: it begins to rise if it has been falling, or to fall if it has been rising.

The turmoil that goes on within the cycles in aggregate activity has a systematic core. A highly simplified picture of the system is afforded by the accompanying table, which condenses Mitchell's analysis of "comprehensive series" in Chapter 10. The table shows directions of movement during a typical business cycle – here divided into eight segments, four each for expansion and contraction. Of course, each segment includes several months, and the table is therefore insensitive to minor differences in timing, such as the short lag in income payments. Further, it hides many crosscurrents that would appear in less comprehensive series, and omits certain business factors of which we should take account – especially wage rates, inventories, banking, and governmental finance. But with all its faults, the table gives an effective glimpse of the typical round of developments that constitute a business cycle.[8]

Let us then take our stand at the bottom of a depression and watch events as they unfold. Production characteristically rises in the first segment of expansion; so does employment and money income; and so do commodity prices, imports, domestic trade, security transactions. Indeed, every series moves upward except bond yields and bankruptcies. In the second stage the broad advance continues, though it is checked at one point – the bond market where trading begins to decline. Bond prices join bond sales in the next stage; in other words, long-term interest rates – which fell during the first half of expansion – begin to rise. In the final stretch of expansion, declines become fairly general in the financial sector. Share trading and stock prices move downward; the liabilities of business failures, which hitherto have been receding, move up again; security issues and construction contracts drop; the turnover of bank deposits slackens; and bank debits in New York City, though not as yet in the interior, become smaller.

[8] This and the three following paragraphs are adapted from the National Bureau's *Thirtieth Annual Report*.

Characteristic Direction of Twenty-six 'Comprehensive' Series during a Business Cycle[a]

Series	Expansion: *Trough to first third*	Expansion: *First to middle third*	Expansion: *Middle to last third*	Expansion: *Last third to peak*	Contraction: *Peak to first third*	Contraction: *First to middle third*	Contraction: *Middle to last third*	Contraction: *Last third to trough*	No. of Business Cycles Covered	% of Conforming Movements during Span of Stages in Which Series Is Said to *Rise*	% of Conforming Movements during Span of Stages in Which Series Is Said to *Fall*
Bonds sold, N. Y. Stock Exchange	+	−	−	−	−	−	+	+	14	86	79
R.R. bond prices	+	+	−	−	−	+	+	+	19	65	74
Business failures, liabilities, *inv.*	+	+	+	−	−	−	+	+	14	86	100
Common stock prices	+	+	+	−	−	−	−	+	16	94	82
Shares sold, N. Y. Stock Exchange	+	+	+	−	−	−	−	+	16	94	88
Corporate security issues	+	+	+	−	−	−	−	+	8	100	75
Construction contracts, value	+	+	+	−	−	−	−	+	7	86	75
Deposits activity	+	+	+	−	−	−	−	+	16	94	88
Bank clearings or debits, N.Y.C.	+	+	+	−	−	−	−	+	18	100	89
Incorporations, no.	+	+	+	+	−	−	−	+	19	84	80
Bank clearings or debits, outside N.Y.C.	+	+	+	+	−	−	−	+	14	100	79
Bank clearings or debits, total	+	+	+	+	−	−	−	+	14	100	93
Imports, value	+	+	+	+	−	−	−	+	16	94	75
Industrial production, total	+	+	+	+	−	−	−	−	5	100	100
Fuel & electricity production	+	+	+	+	−	−	−	−	5	100	100
Pig iron production	+	+	+	+	−	−	−	−	16	100	100
R.R. freight ton miles	+	+	+	+	−	−	−	−	9	100	89
Factory employment	+	+	+	+	−	−	−	−	6	100	100
Factory payrolls	+	+	+	+	−	−	−	−	5	100	100
Income payments, total	+	+	+	+	−	−	−	−	4	100	50
Corporate profits	+	+	+	+	−	−	−	−	4	100	100
Business failures, no., *inv.*	+	+	+	+	−	−	−	−	16	75	88
Department store sales, deflated	+	+	+	+	−	−	−	−	4	100	75
Wholesale trade sales, value	+	+	+	+	−	−	−	−	3	100	100
Wholesale commodity prices	+	+	+	+	−	−	−	−	11	82	91
R.R. bond yields	−	−	+	+	+	−	−	−	19	74	65

[a] Derived from pp. 256-7. A plus denotes rise, a minus denotes fall. The two series on failures are inverted here. Bond prices are treated as the inverted replica of bond yields; see p. 262, note *e*, concerning their sign in the second segment of contraction.

These adverse developments soon engulf the economic system as a whole, and the next stage of the business cycle is the first stage of contraction. Production, employment, commodity prices, personal incomes, business profits – indeed, practically every process represented in the table declines. Of course, the liabilities of business failures continue to rise, which merely attests the sweep of depression. Long-term interest rates also maintain their rise. But in the next stage the downward drift of bond prices ceases; that is, the rise in long-term interest rates is arrested. By the middle of contraction, bond sales join the upward movement of bond prices. More important still, the liabilities of business failures begin declining, which signifies that the liquidation of distressed business firms has passed its worst phase. These favorable developments are reinforced in the following stage. Share trading and prices revive; business incorporations, security issues, and construction contracts move upward; money begins to turn over more rapidly; even total money payments expand. Before long the expansion spreads to production, employment, prices, money incomes, and domestic trade. But this is already the initial stage of general expansion – the point at which our hurried trip around the business cycle started.

Of course, this recital delineates characteristic movements during business cycles, not invariant sequences. That the description fits imperfectly individual business cycles is apparent from the conformity percentages in the table. Yet these percentages also suggest that the deviations from type are not so numerous as to destroy the value of a generalized sketch. And if this much is accepted, an important conclusion immediately follows, notwithstanding the omissions of the table; namely, that the check to the dominant movement of business activity, whether it be expansion or contraction, is typically felt especially early in financial processes and activities preparatory to investment expenditure.

The contraction phase of business cycles is not, however, the precise counterpart of expansion. This is clear from the table and becomes clearer still when numerical values are attached to its signs and intervals. The arrays of individual turning points at business-cycle troughs "are more dispersed and skewed toward leads" than are the arrays at peaks. Expansions of aggregate activity average longer than contractions. They are also more vigorous, so that the trough from which a given expansion starts is ordinarily above the level from which the preceding expansion started. In the first segment of expansion the rate of improvement "is more rapid than at any other stage of the cycle". A "sharp and general retardation" of the advance occurs in the next segment. In the third, while "reacceleration is the rule", the advance "does not regain the speed" it had at the beginning of expansion. In the final stage of expansion "the business tide

. . . becomes fuller of eddies". Contractions follow a different pattern. "The fall accelerates somewhat in the second segment of contraction, whereas the rise is much retarded in the second segment of expansion." The next stage "brings a moderate retardation" of the decline, whereas it "brought a moderate reacceleration" of the advance. The closing stages of expansion and contraction are similar "in that the rate of change becomes slower; but this retardation is much more marked at the end of contraction than at the end of expansion".[9]

> Thus the notions often suggested by the picturesque phrasing beloved of writers upon 'booms and busts' – that prosperity grows at a dizzier pace the longer it lasts, and that slumps gather momentum as they proceed – are wrong if our measures are right. Scarcely less misleading are the implications of the mathematical constructions often used to represent business cycles. A set of straight lines sloping upward to represent expansion, connected at a sharp peak with downward sloping straight lines to represent contraction, misrepresents the facts. . . . Sine curves are not less objectionable. . . . What our observations suggest is that the shapes of business cycles are phenomena *sui generis* (p. 300).

V

These, then, are some of the broad results that emerge from Mitchell's examination of the cyclical process of the American economy. The full range of the book, its suggestions for further research, and its exemplary scientific care await the reader. Economists anxious to wield a simple formula of the causes of business cycles or the means of controlling them will not find Mitchell's fragment to their liking. Those willing to take conclusions on faith may chafe at its patient elaboration of evidence. But men who seek so earnestly to understand how our economic organization works that they insist on judging evidence for themselves are more likely to lament that too much detail has been suppressed. Scholars will respect Mitchell's pronouncement that his report on findings, after many years of research, is "ill proportioned, tentative, and subject to change as the investigation proceeds" (p. 5).

This book is not easy and everyone will save time by a careful reading of Part I which, besides outlining aims and methods, provides the modicum of technical vocabulary required for comprehending what follows. Economic theorists are likely to find especially suggestive Chapter 7, which sets out the facts and inquires into the causes of the changing proportions among economic quantities in the course of a business cycle; also Part III, which centers on the consensus of fluctuations in leading sectors of the economy. Chapter 8 is a useful reminder to all that, despite their persistent traits, business cycles are changing phenomena; and that

[9] See pp. 75, 299-305.

just as each new member of a group has traits of his own, which cannot be inferred from knowledge of the 'average man', so each business situation must be judged in the light of its own circumstances as well as according to historical patterns. The bulk of this chapter is devoted to technical problems in the decomposition of time series, and only specialists will want to study it fully. Readers pressed for time might move lightly through Chapters 5 and 6 also, except for the closing sections which will repay careful reading.

The modern theory of employment, which for a time pushed aside both value and business-cycle theory, is now slowly being fitted into older economic knowledge. The younger economists are rediscovering that cost-price relations play a significant role in shaping the national income and its movements, that the 'consumption function' itself moves cyclically, that investment is not an autonomous variable, that price inflation does not wait for full employment, and that both investment and consumption are heterogeneous aggregates that cannot be understood without separate analysis of their parts. If our harassed generation can win the opportunity to pursue the arts of peace, the fruit and example of Mitchell's work will have their quiet but decisive part over the years in bringing the theory of fluctuations into ever closer contact with the ebb and flow of experience.

COMMENT

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Empirical economics is at its very beginnings. Without Wesley Mitchell's faith and encouragement it would not have made even the little progress achieved so far. One possible contribution towards continuing this progress is to help in gaining a clear idea of Mitchell's own propositions. We must translate them, if necessary, into terms that are as unequivocal as possible and that might yield themselves to well defined, or definable, empirical tests. After sketching such a tentative translation, I shall discuss the types of information that were or could be employed to either state or test hypotheses on business cycles. In doing so, I shall give especial attention to the role which general insight into human behavior and institutions plays in Mitchell's posthumous piece of work, the way in which such insight might be best combined with statistical time series, and the use that can be made of the knowledge so combined.

I

When, in Chapter 2 of the *Progress Report,* Mitchell tries to "develop a working definition of business cycles" he states, in effect, a hypothesis – the hypothesis that such cycles exist: "Business cycles are a type of fluctuations *found* [my italics] in the aggregate economic activity of nations that organize their work mainly in business enterprises." The definition and the subsequent text (Ch. 2 and 3) go on to describe properties of this type of fluctuation. To prepare the ground for a formal restatement of these rather complicated properties we may first give the hypothesis a deliberately simplified form. It is admittedly not Mitchell's; but it helps to introduce some of the relevant concepts. Our simplified hypothesis is:

> In a society consisting mainly of business enterprises, and apart from seasonal and small random influences, certain economic variables are periodic functions of time; the period length is the same for all these variables; for each variable, each period contains exactly one peak and one trough. (It follows that for any two variables the distance in time [the 'lag'] between the corresponding peaks is constant.)

The phrase *apart from random influences* that we have just used is often encountered in business cycle literature. We must define it and thus get it out of our way. It seems that a statement $S\ (a, b, \ldots)$ made about the variables $a, b, \ldots$ is usually called "true apart from random influences" if there exists a true statement $S^*\ (a, b, \ldots; u, v, \ldots)$ about $a, b, \ldots$ *and* about some, generally unobservable, random variables $u, v, \ldots$, such that, if $u, v, \ldots$ vanished, S^* would become identical with S; symbolically, $S\ (a, b, \ldots) = S^*\ (a, b, \ldots; O, O, \ldots)$. As to the term "random variable" itself, it is used to describe any variable such that a fixed probability can be assigned to the occurrence of any specified range of its values. The more special phrase "S is true apart from *small* random influences" should also be defined, but I shall not try to do so here. Lack of time forces me also to assume that the phrase "apart from seasonal influences" has the same precise meaning for everyone, although this is probably untrue. Unfortunately, this omission is not altogether trivial in our context: since Mitchell often operates on data for periods shorter than a year, his definition of seasonal influences, necessarily implied in his method of eliminating them, may affect the conclusions.

Mitchell's actual hypothesis is more complicated than the one I have stated. It is weaker inasmuch as the constant period length is replaced by a variable (possibly random) period length, restricted, however, in the case of the United States, by certain specified properties. Here is one tentative formulation:

> In the United States, and apart from seasonal and small random influences, there exists a sequence of intervals that is common to all economic variables

and has the following properties: (1) each interval lasts from 2 to 12 years and overlaps its successor by 3 months; (2) each interval contains, for each variable, exactly 3 turning points, of which 2 are near the ends of the interval; (3) the time lag between two nearest peaks (troughs) for any two variables retains its sign (+, −, 0) throughout the sequence of intervals; (4) this time lag is zero for a large proportion of pairs of variables.

The style of Mitchell's presentation does not make it easy to discuss whether this translation is faithful. Supposing it gives the gist of his findings, it is not easy to define and discuss the tests (or the estimation methods) used. Let us assume that the translation is correct in essence, and grant that the tests are valid and the estimates (lengths of intervals and lags) are satisfactory for practical purposes. Notice that the hypothesis thus accepted is implied in any of a number of hypotheses about some 'mechanism' generating the observed set of economic time series. One such 'mechanism' is this:

> There exists a variable (called 'general business activity') whose value at any point of time depends, apart from random influences, upon one or more of its preceding values. (Such a function of time is called a 'stochastic process'.) Apart from random influences, each observable economic variable is a certain, possibly lagged, function of the general business activity.

Tentatively, I think it is justifiable to say that Mitchell implicitly accepted this last hypothesis. The operations actually proposed by Mitchell to derive the general business activity for a given point of time consist essentially in expressing each variable as a relative deviation from its observed average over time; in finding lags by direct inspection of turning points; and in averaging the properly lagged relative deviations over all variables for a given point of time. This suggests that the hypothesis in the investigator's mind was even stronger than the one just stated. For example, a possible justification for those operations might be the following hypothesis: each variable, adjusted for its characteristic lag and divided by its average, is proportional to "general business activity", apart from an additive random term, whose average (mathematical expectation) over time for a given variable is zero, and whose average over all variables for a given time is also zero. It is not denied that some alternative proposition might be constructed that would lead to the same operations. But many passages of the book suggest, in fact, that Mitchell often regarded each properly adjusted and calibrated time series of economic variables as a drawing (not necessarily an independent one) from a random universe of which the "general business activity" is the mean. In addition, he often expresses himself as if he regarded each cycle (the 2 to 12 year time intervals) as a drawing from a random universe, or, in his words, a "unit of a species". Provided these "units" are not assumed to be mutually inde-

pendent, this view is consistent with the hypothesis that "general business activity" constitutes a stochastic process; although the cutting up of the process into intervals defined by selected turning points is probably not the most efficient start for estimating its properties. Moreover, it is not clear from actual arithmetical operations whether our proviso, that the successive cycles must not be treated as independent drawings, was fully heeded. If it was not, the mechanism assumed must have been a more special one than the one I stated tentatively.

However, the existence of a "general business activity" of which the observed economic variables are random and lagged functions is only a sufficient, not a necessary condition for the validity of Mitchell's finding of 2 to 12 year intervals, possessing certain properties with respect to economic variables. Among many other mechanisms that would imply the same properties of the observed time series, the following simple ones may be mentioned: The observable variables are mutually connected by a number of lagged relationships, apart from random influences (such is the familiar 'cobweb'). Or: Apart from random influences, the observable stocks of some commodities depend on certain variables which in turn depend on the rate of change of the flows of those commodities. In listening to this, you are rightly thinking of dynamic econometric models, systems of differential or difference equations. At this point, I do not argue their advantages.

To be sure, Mitchell does state that "general business activity" is merely a "nebulous congeries". It is a formal construct, a unifying index. We must be clear, however, that such an index does not add anything to our knowledge unless its changes are indeed assumed to govern the changes in the economic variables that interest us. (This was well discussed in Keynes' *Treatise* of 1930, when he criticized Jevons and his followers for pursuing the mirage of a 'true' general price level.) To change the field, suppose for a moment that pulse, temperature, blood pressure and composition, metabolism, etc. were each a function (apart from random influences) of the same variable – the 'general health' of the individual. The pulse, the temperature, etc. would then indeed reflect, every one of them, the state of health; and an appropriate average ('index') compounded from those measurements would reflect it still better, by virtue of reducing the random influences. As a matter of fact, however, those measurements are never compounded into a single index, because they are never thought of as reflecting a single 'general health'. Instead, a physiological theory consists of relations between a number of variables, some of which are more easily measurable than others. If these relations are known, the values – current or future – of variables that are not easily measurable but are deemed important may be estimated from a set of easy current

measurements, such as temperature taking. The physiologist tries to improve his knowledge of those relations, and the practitioner uses them.

II

Similarly, the economist should try to improve his knowledge of relations among economic variables. From what sources? Economic time series are not our only source. Astronomical time series were not the only source for Newton's explanation of planetary motions. His other source was the experiments, such as Galileo's, on falling bodies. And either source would remain silent if he had not asked it to confirm or reject his basic hypothesis, the law of gravity. In the economist's case, time series are supplemented by the knowledge of responses of individual households and firms, by the knowledge of human institutions and technologies; and we have to approach all those sources with some hypotheses in mind. (Mitchell was doubtless aware of this, yet he often seems to recommend the use of hypotheses on human behavior and sources other than time series merely as a check, an afterthought, a 'this-is-what-we-should-expect'.)

When you survey New York from the top of the Empire State Building, you see a procession of beetles, moving, stopping, moving, stopping. After a few glances at the street and at your watch, you will obtain a good 'cyclical pattern' of stops and moves. If, in addition, you pay attention to colors, you may find that the proportion of red to green beetles in any given section of the avenue also fluctuates in time, though probably with less pronounced cyclicity; but that the occurrence of red and green lights at certain points of the avenue is indeed rigidly cyclical and is strongly correlated (almost without a lag) with the stops and moves of the beetles at these same points. Solid empirical knowledge! But how much more certain and useful it would become if you knew, in addition to your time series, another set of facts: that both the beetles and the lights are controlled by men like you; that few auto drivers have reason to drive in reverse; and that there exists a certain agreement (an institution) between the drivers and those who control the lights. Thus fortified, the knowledge of the tower watchman becomes more certain because he can foresee the circumstances under which the cycles may change their character, e.g., become longer at night. His knowledge becomes more useful also because he will now stop studying the color composition of vehicles, as irrelevant; and because he will be able, instead, to make sensible recommendations from his vantage point. For example, knowing now that the lights determine the moves of beetles (and not conversely) he may recommend a change in the rhythm of lights so as to ensure smoother traffic; or, if he

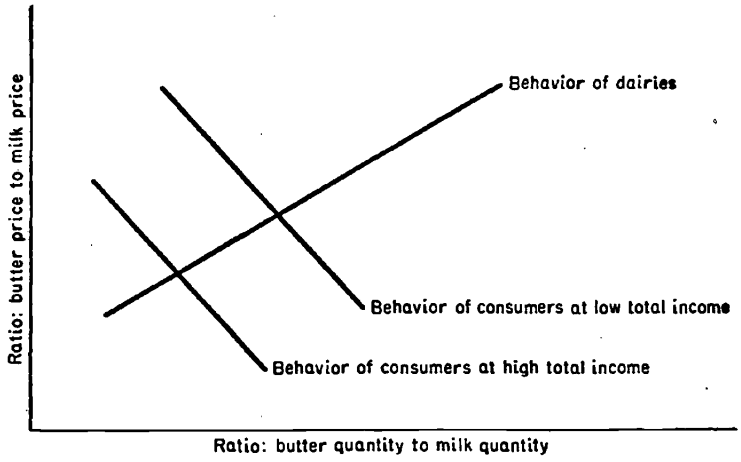
has friends among drivers, he may tell them what speed they should maintain to miss red lights all along the avenue. He can thus be likened to the economist called upon by the government or advising a single firm. Of such an economist it is often said that, in addition to generalizations drawn from empirical time series he has used 'economic theory'. This is an unfortunate terminology, suggesting a false distinction between theory and empirical knowledge. I prefer to say that economics becomes more secure and more useful if, in addition to time series, we use all other available information, including our insight into the plausible behavior of human beings, especially when they are our fellow-countrymen and contemporaries, and our knowledge of human institutions and techniques.

Mitchell saw the need for using our knowledge of human behavior and institutions. But one may question whether he used it, or intended to use it, in the most efficient way. After a frankly mechanical, though time-consuming, manipulation of the 800 observed series, he interprets the results – the lags, the comparative amplitudes – often to state that "none of the findings seems surprising" (Ch. 7). Sometimes the check is performed on mere identities and is therefore essentially a check on the consistency of data – as when the series of wage rates and weekly working hours are compared with their product, the series of weekly earnings. When, on the other hand, the empirical results were checked not against identities but against verifiable implications of plausible human behavior, these implications were seldom thought through exhaustively – either because time ran short or, more likely, because Mitchell genuinely underestimated the logical intricacy of the economic system. In Mitchell's words (Ch. 5):

> When the analysis is written out in full it becomes a long chain of commonplace observations upon familiar experiences. Department stores sell more goods in expansion because income payments to individuals have increased. Less butter is produced because families buy more milk as their incomes rise; the flow of milk cannot be increased as rapidly as consumption grows, so less milk is turned into butter. Essentially similar reasoning can be extended to all sectors of economy.

The butter example shows that more careful reasoning would be necessary. Must a rise in income lead necessarily to an increase in milk consumption and a consequent fall in butter consumption (the total flow of milk being constant for physical reasons)? Why not to a rise in butter consumption and a fall in milk consumption? It is worth while to pause here. As a simple hypothesis, let us grant that the total milk flow is a technological datum, subject to slow and negligible changes only. Dairies will change the ratio of butter to milk in response to the changing ratio between the price of butter and of milk – presumably increasing the former

ratio if the latter ratio increases. Given a family's real income, number of children, etc., the ratio of its butter demand to its milk demand will presumably fall as the ratio between the two prices rises. Hence for a given aggregate real income (and its distribution by income, family size, etc.) we can plot both the quantity-ratio supplied and the quantity-ratio demanded against the price-ratio (see the graph).



As real income rises, the 'demand curve' will shift – but in which direction? In other words: does increased income induce consumers to raise or to diminish their demand for butter relative to milk? Mitchell's empirical result implies that they will reduce it. But could this answer be obtained also from "commonplace observation on familiar experiences"? I think not. There would be nothing incompatible with common sense if the consumers responded in the opposite way. Contrary to his own opinion, Mitchell's result – that milk consumption grows and falls with income – is *not* trivial. As we have seen, it appears trivial only if we refuse to puzzle over the human responses and institutions beneath the surface of economic figures. Systems of simultaneous relationships tentatively describing the economic intercourse (and exemplified, albeit in an unduly simple form, on our graph) suggest also ways for checking a given empirical result with other data. In our case, for example, one sees the importance of bringing in additional time series, viz., butter and milk prices (their ratio should fall with rising income if the hypothesis is correct); one sees how the data on family budgets and the production records of dairies, both commercial and experimental, might be used. This is not all. If we succeed in formulating our findings in terms of numerically specified human responses, e.g., if the demands for milk and butter are expressed as numerical functions of

income and the prices, predictions can be made about the effect of institutional or technological changes: we can estimate the effect of artificial price fixation, say, or of a change in freight or tariff rates, etc. Such questions cannot be answered if we content ourselves with the finding that milk consumption rises with income. It is in this sense that prediction presupposes explanation.

As a second example of Mitchell's attitude to our knowledge of, or hypotheses on, human behavior, I shall use his section, Reference-Cycle Amplitudes of Prices and Production (Ch. 7). He writes:

> We know that increases in supply tend to depress prices and that increases in demand tend to raise them; but how will prices behave in a cyclical expansion when both supply and demand rise, or in a contraction when both supply and demand shrink? It is in this theoretically indeterminate form that price problems confront students of business cycles. What to expect we learn from experience: most prices rise and fall with the cyclical tides of business activity most of the time – not always.

In this case, unlike that of milk consumption, knowledge of human behavior that we may have independently of time series is recognized by Mitchell as indeterminate with regard to the question asked. Hence, the empirical findings cannot be trivial. But his empirical analysis does not seem to me to have been organized in a way that would most efficiently supplement the knowledge of human behavior by available time series or other statistical data. The first sentence of the passage just quoted suggests that the demand curve and/or the supply curve (each expressing the response of a specified social group to a change in price) are shifted by changes in consumers' income, profit expectations, etc.; changes that are reflected, although only imperfectly, in the general index of business activity, and hence in the position of the observation time-point on the 'reference cycle' axis. An effort might be made to purify the index from irrelevant components. For example, one might replace it, in the case of the demand for food, by variables deemed to determine consumers' decisions: for example, real income, but hardly carloadings or bond rates. For the demand for steel or cement, or for the supply of food, other determining variables would be chosen. One would then attempt to estimate, for a given commodity or group of commodities, the dependence of demand on price (the Marshallian demand curve) as well as on other factors relevant to buyers' decisions (and causing measurable 'shifts' of the Marshallian demand curve). If similar attempts were made on the supply side, one would obtain an explanation of the changes of both price and quantity of the commodity through time, and in relation to other variables. For example, suppose the supply curve is positively sloped and the demand curve negatively sloped; and suppose we have found from various data

that, in response to changes in real income, in profits, and in other variables concealed in the general business index, the demand curve reacts by stronger shifts than the supply curve, e.g., that at a given price, a billion dollar change in income raises the quantity demanded by a million bushels and the quantity supplied by only half a million bushels. Then we would have explained the direction, and even the size, of the changes that the price of the commodity, and its quantity bought and sold, undergo in response to given changes in the relevant components of the business index, and hence (a less interesting question) in the index itself. To obtain this explanation, time series had to be combined with additional knowledge; hence the explanation would be, in general, more secure than if we had used time series only. The explanation would also be more useful in that it would enable us to predict the results of given changes of the assumed institutions or technologies. For example, a given invention or an excise tax will change the supply curve in a specifiable fashion; the redistribution of income in favor of the poor would affect the demand curve in a certain way; price fixation by government removes the equality between quantity demanded and quantity supplied, and so on. If the system of economic relationships valid under old institutions and technologies was properly estimated, and if the assumed change in institution or technology is well defined, the resulting change in the prices and quantities can be estimated in advance.

In his empirical analysis of prices and quantities Mitchell, following F. C. Mills, takes a different route. He makes little use of the knowledge of human behavior contained in the first sentence of his own statement quoted above. Instead, he counts the number of price or production series that rise or fall with a rising business index or, in another terminology, have 'positive' or 'inverted' timing; (there are also series with 'neutral' or 'irregular' timing). The cases are counted separately for contraction and expansion periods. The counting is done separately also for farm products and for other products; for goods with administered prices and other goods; and for durable, nondurable, and semidurable goods. This classification, based on knowledge of technology and institutions, is no doubt significant. But it is not followed by a corresponding purification of the business index – the "nebulous congeries" – to exclude factors irrelevant to the particular commodity group; more important still, the classification of goods is not followed by the distinction between the decisions of producers and those of consumers (and possibly of others, such as dealers or lenders), decisions that are certainly determined by different sets of factors and not by the same "nebulous congeries". The method of counting signs, instead of operating on the amounts, of changes of variables, is probably an unnecessarily coarse, hence inefficient way of using

available figures, even if one takes account of its labor saving advantages; and when these signs cannot be regarded as independent drawings from a homogeneous universe, counting may lead to biased inferences. Finally, even supposing that all inferences were correct and that a certain true pattern describing the simultaneous changes of the business index, the price of the commodity and its quantity, were discovered for the past, what use could be made of this knowledge? In the future the three variables just mentioned will be observed simultaneously so that the need for predicting one variable from another, or from the other two will not arise -- apart from the special case when, for external reasons, the observation on one variable becomes available, e.g., published, earlier than on another one. More important, the pattern does not enable us to predict how a given, well specified change in institutions or technology would affect any of the variables studied in the past; for such prediction requires the knowledge of demand and supply curves (and of their shifts under given changes in income, etc.) under the old conditions, and the specification of the institutional or technological change in terms of such curves (and of their shifts).

Institutional changes have, of course, taken place also in the past. In fact, some are listed among the 800 time series used by Mitchell; for example, this list includes several series on public construction work. These, says Mitchell,

> have irregular timing, because governments do not build for profit to themselves. Just why they time the letting of contracts . . . as they do is a question that has many answers – pressing needs for more schoolrooms, sanitary requirements, . . . local pride, political jobbery, and so on.

This means that, in another terminology, public construction works are "exogenous variables": they are not determined by, though they may determine, economic variables. Most likely, various other aspects of the governmental fiscal and monetary policy, tax rates and expenditures, the monetary gold stock, tariffs, supported bond prices or farm prices, the volume of government stored products, arbitrated wage rates, the size of the armed forces, are exogenous or at least are partly determined by exogenous variables. Exogenous variables may be regarded as parameters, the 'givens', of the economic system that have undergone observable changes in the past. Other parameters may not have changed in the past. All parameters of the system may undergo changes in the future. The practical problem is to predict the effect of a given change in parameters upon the economic variables; for example, to predict the effect of increasing armaments, decreasing a tax rate, or introducing or removing price control.

Mitchell discusses whether to exclude public construction works, admin-

istered prices, and similarly 'irregular' time series when computing the index of general activity, and decides that to do so would distort the facts. To me the question itself seems irrelevant but it helps to understand Mitchell's general attitude towards economic facts and policies. It seems to me that a wave pattern that can describe American business cycles at a time when public expenditures equalled a tenth of national income is unlikely to have much predictive value for a time when public expenditures equal a fifth or a fourth of national income; nor can it help to predict how a given change in public expenditures or in administered prices and wages, etc. can affect national income or any other economic variable. To Mitchell, effects of changes in these or similar parameters appear negligible, thus enabling his "cyclical procession" to roll on majestically like the planets. Although Mitchell does occasionally urge (Ch. 7) that the nation needs "economic 'know how' ", viz., "to use its resources at tolerably full capacity year in and year out", he is not particularly concerned with how policies have affected or can affect the course of economic time series; nor is it clear how his analysis might help to answer such questions.

Perhaps we face here a difference between generations. Mitchell's thought is in the grand manner of the masters of the ninteenth century, including Marx: economic laws of the capitalist society are inexorable (as long as it exists), with little place left for deliberate changes. If this were true, there would indeed be no need for looking beyond the surface of the recorded wave-like movements; suffice it to know – and this merely as a matter of curiosity – that they will repeat themselves. But if those movements do depend on human institutions, they will change in response to institutional changes. To find how these movements change their character, we have to study more closely how economic variables are determined. But to this question, time series alone give no answer; other knowledge, in particular that of human decisions and institutions, has to be added. Thus, our interest in combining time series with 'economic theory' goes hand in hand with our belief that economic laws, far from being inexorable, depend on habits, institutions, and policies. Perhaps this generation has learned it from the experience of recent upheavals. "To find out how business cycles come to differ from one another as much as they patently do . . . remains for later times and other hands", says Mitchell. It is in keeping with the memory of a courageous and persistent thinker that those "other hands" try to dig deeper, in order to find more.

REPLY BY MR. BURNS

1) Since Professor Marschak has devoted the first part of his paper to Mitchell's definition of business cycles, it may be well to recall the definition and its place in Mitchell's work. The definition has a long history. Mitchell's *Business Cycles: The Problem and Its Setting,* published in 1927, is essentially a summary of what he had learned during the preceding five or six years in an effort to equip himself for further constructive work on business cycles. Not until the last chapter, where he sketched his "working plans", did Mitchell present a definition of business cycles. Its purpose was to guide future research. This "working definition", with some modifications for which I was responsible, is repeated in *Measuring Business Cycles,* published in 1946. Let me quote it:

> Business cycles are a type of fluctuation found in the aggregate economic activity of nations that organize their work mainly in business enterprises: a cycle consists of expansions occurring at about the same time in many economic activities, followed by similarly general recessions, contractions, and revivals which merge into the expansion phase of the next cycle; this sequence of changes is recurrent but not periodic; in duration business cycles vary from more than one year to ten or twelve years; they are not divisible into shorter cycles of similar character with amplitudes approximating their own (p. 3).

The definition was followed by a long discussion explaining that it was a tool of research, that it raised numerous questions of fact, and that the concept of business cycles it embodied was itself on trial.

Mitchell recalls once again in *What Happens during Business Cycles* the instrumental character of the definition:

> This definition stated our preliminary notion about the cultures in which business cycles appear, and described what we thought to be the common features by which they can be recognized and distinguished from movements of other types. Whether the definition would serve its purpose could be determined only by using it as a guide to observation. Do cyclical fluctuations of the alleged duration occur in most economic activities? If so, is there substantial agreement in their timing? What leads and lags appear? If a dominant pattern emerges, what activities share in it and what follow different courses? Do cyclical expansions and contractions run a continuous round, or is one cycle sometimes separated from its successor by an interval during which the tide neither ebbs nor flows? Do business cycles occur in all nations where private enterprise prevails, and only there? How far back can they be traced in history? What relations subsist among the cycles in different nations? (p. 7)

Mitchell proceeded to indicate how a scheme of measurement was evolved to cope with these and related questions. And with these explanations out of the way he plunged into a report on findings. He tried to make it very clear, first, that he interpreted the evidence accumulated by the National Bureau to mean that business cycles are not merely fluctuations of some

sort, but that they have the characteristics set out in his definition; second, that he had found historical instances of the species within the dates marked off by the Bureau's business-cycle chronology; third, that he felt he had succeeded in replacing the vague clauses of the definition by concrete and fairly dependable generalizations. Mitchell's belief that his working concept had been validated stands out boldly in *What Happens during Business Cycles.*

Now, I think it is highly important that students of business cycles appraise with a sceptical eye Mitchell's findings. How successful was he in adding to knowledge of business fluctuations? Did he fall into serious error and if so at what points? Are any of his findings so vague that they cannot be properly tested? If so, what are they? Which of his results are dependable and which are not? And which of the former, if any, have large implications for new, as yet unknown, situations? These are the important matters from the viewpoint of science. By using Mitchell's definition as a handle for such a discussion, Marschak might have rendered valuable scientific aid to other workers in the field.

Instead, Marschak has expended effort on what seems to me to be a sterile exercise in translation. That his rendering of Mitchell's definition is not faithful to the original will, I think, be clear to anyone who takes the trouble to compare the two. If the translation has any advantage over the original, whether from the viewpoint of testing or any other, Marschak has not pointed it out. The real difficulty, I believe, is that Marschak fails to see the instrumental character of Mitchell's definition. He thinks of it as a conclusion of Mitchell's research, whereas it was virtually the beginning, having been designed to guide systematic observation of business cycles. Taking the definition (or rather, his translation of it) as a conclusion, he goes on to say that a number of mechanisms could generate the phenomena set out in the definition, and he specifies a "mechanism" which he believes Mitchell had in mind. But Marschak is quite wrong in the hypothesis he attributes to Mitchell. Mitchell saw the essence of the business cycle in a systematic *divergence* of the movements of different parts of the economy, not in the movements of some composite curve. The notion that "there exists a variable (called 'general business activity') whose value at any point of time depends, apart from random influences, upon one or more of its preceding values" does not belong to Mitchell's universe of thought – in *What Happens during Business Cycles* or in any other of his writings. True, he viewed each stage of the business situation as evolving into its successor, but by this he always meant that the *relations* existing at any one time among the parts of our interdependent system shaped the general direction of activities at later times. Nor did Mitchell believe that, "apart from random influences, each observable economic variable is a

certain, possibly lagged function of the general business activity". As my Introduction to Mitchell's book points out, he made a considerable effort to demonstrate that numerous activities, some of large significance, move independently of business cycles.

2) I shall now try to clear up a point that apparently misled Marschak in the course of his translation and that causes trouble in the second section of his paper as well as in the first. To observe, as Mitchell did, that most activities move in varying degrees of conformity with a reference scale of business cycles, and therefore with each other, is not to say or to imply that each bears a definite causal relationship to "general business activity". It simply means that the business cycle is a special kind of fluctuation in aggregate activity – a fluctuation that is widely diffused through the economy. Analysis of the process of this diffusion was uppermost in Mitchell's mind. Naturally, this requires tracing the causal relations among specific activities, and the National Bureau's elaborate statistical apparatus -- the reference scale of business cycles and the various measures of specific and reference cycles – was devised to arrange facts so as to facilitate such inquiries.

Marschak has not grasped this fact. He labors under the impression that Mitchell's design for tracing causation involved solely, or mainly, analysis of the relation that each economic variable bears to the "nebulous congeries" – general business activity. He believes that in carrying out the plan an index of "general business activity" was constructed, and he even attempts to describe how that was done. But all this is fanciful. There has never been any such design or any such statistical construction in the National Bureau's scheme. Marschak's interpretation being what it is, it is hardly surprising that he sees a need to suggest that, in analyzing the demand for food, one should concentrate on the influence exercised by real income rather than on the influence of carloadings or bond yields or general business activity. He is right, of course; but there is no difference here between him and Mitchell.

What the sources of Marschak's misapprehension may be I cannot be sure. I shall put down, however, several remarks that may perhaps clarify Mitchell's work. First, what the National Bureau has developed is a reference scale of business cycles, not an index of general business. The scale simply consists of a succession of dates when economic activity at large appears to have reached a peak after a sustained upward movement or a trough after a sustained downward movement. Second, this reference scale facilitates systematic comparisons of the directions and rates of change *among* particular economic variables. By laying out economic series on this Procrustean scale, the sequence of cyclical turns in related activities can be readily made out, and so also their rates of movement

during historical intervals that are supposed to be in a similar (or different) business-cycle phase. Third, Mitchell's findings would not be significantly altered if his reference scale, which is derived from numerous economic series, were replaced by a scale – as far as this is at all feasible – derived from a single comprehensive series such as national income or employment. Fourth, although there is more causal analysis in Mitchell's book than hits the eye, he did not even complete his descriptive account of how various economic activities have behaved during the periods marked off by his reference scale. In other words, he did not finish laying the factual groundwork for attacking the central theoretical problem as he saw it; namely, "how an economic system of *interrelated* parts develops *internal* stresses during expansions, stresses that bring on recessions, and how the *uneven* contractions of its varied parts pave the way for revivals" (p, 295, my italics).

3) Before turning specifically to the second section of Marschak's paper, I should make one more remark about Mitchell's book. Despite the considerable statistical detail, it focuses on the broad features of business cycles. Individual processes, commodities, even industries are treated incidentally. The following quotation illustrates his general approach:

> In trying to account for the wide variety of reference-cycle amplitudes . . . we might take up one series after another, *considering in each case the conditions under which the activity represented is conducted, the other activities by which it is most influenced, and their behavior as factors shaping the behavior of the activity on which we are focusing attention for the time being.* That is the procedure to follow whenever one is concerned with the cyclical amplitudes of particular series; but it is ill adapted to a systematic survey because of its mountainous detail. For the present, we can get on faster by asking, not why steel production has large reference-cycle amplitudes and department store sales much smaller ones, but by asking what factors tend to produce large and what tend to produce small fluctuations (p. 113, my italics).

Mitchell pointed out time and again the limitations of his "progress report", and called attention to the Bureau's monographic studies in which the factors that dominate particular activities or markets are analyzed in some detail. Unless this fact is borne in mind, the reader will not see the second section of Marschak's paper in a just perspective.

The central theme of this section, as I read it, is Mitchell's attitude toward economic theory. Marschak apparently feels that Mitchell concentrated unduly on the behavior of time series, that he gave too little attention to "the responses of individual households and firms", that he rather neglected knowledge of "human institutions and techniques", and that he "underestimated the logical intricacy of the economic system". Although I cannot recognize Mitchell from Marschak's portrait, I shall not detain the reader at this point by drawing another. I must, however, examine

briefly the illustrations that Marschak presents as evidence for his interpretation.

His major illustration is built around a brief quotation from Mitchell. Marschak singles out this sentence: "Less butter is produced because families buy more milk as their incomes rise; the flow of milk cannot be increased as rapidly as consumption grows, so less milk is turned into butter." In a preceding sentence Mitchell had suggested that what he has to say about milk and butter is a "commonplace" observation about "familiar experiences". Marschak does not like any of this. He feels that Mitchell is not justified in regarding his milk and butter result as trivial: "it appears trivial only if we refuse to puzzle over the human responses and institutions beneath the surface of economic figures". He then proceeds to construct a model which he apparently thinks gives reasonably adequate recognition to "human responses and institutions". Now, I think it is entirely clear that the explanation in Mitchell's milk and butter sentence is incomplete. If he had added one further "commonplace" observation it would have been more complete; namely, that due to imperfections in the market the price of fluid milk is not only rather inflexible over the cycle but relatively high compared with the price of milk used to make butter. But I also fail to find any recognition of this in Marschak's model; in fact his model is not consistent with it. Marschak's supply curve implies a competitive market, in which the price received by farmers is the same whether the milk is used for fluid consumption or for conversion into butter. It completely overlooks the discriminatory pricing so characteristic of the milk industry. Moreover, if the purpose of Marschak's model is to throw light on Mitchell's milk and butter sentence, it blinks another "institutional" datum. What Mitchell contrasted was the consumption of fluid milk with butter production, not butter consumption. Appreciable quantities of butter are held in storage, and they vary decidedly over the cycle. I find no recognition of this, despite its obvious importance for short-run analysis, in Marschak's model.

Marschak has been misled by Mitchell's style which always leans towards understatement. How well I remember Mitchell's bewilderment when he found that butter production had an inverted cyclical pattern, while oleomargarine – of all things – had a positive pattern. This started him on an investigation of the "institutions" of the milk and related industries, and he wrote up the results in a trial monograph he once prepared on the cyclical behavior of production. I cannot blame Marschak for knowing nothing of this, since Mitchell does not refer to it. But Marschak is a little at fault in concentrating his fire on the quoted milk and butter sentence, instead of picking a fuller milk and butter quotation in which prices and inventories are explicitly mentioned (see Ch. 5, Sec. III of

Mitchell's book). He is at fault in not heeding Mitchell's repeated warnings that his comments on specific processes, wherever they may come, are incomplete. And he is at fault in criticizing Mitchell for slighting "the human responses and institutions beneath the surface of economic figures", and yet presenting a model of his own that is by no means free from this defect.

Marschak's second illustration of "Mitchell's attitude to our knowledge of, or hypotheses on, human behavior" draws on Mitchell's comparison of the cyclical behavior of prices and production. As far as I can see, Marschak's principal complaint here is that Mitchell has not worked out numerous demand and supply schedules. Marschak writes with enthusiasm of what could be accomplished with these schedules: "If the system of economic relationships valid under old institutions and technologies was properly estimated, and if the assumed change in institution or technology is well defined, the resulting change in the prices and quantities can be estimated in advance." It is easy to agree with Marschak that if Mitchell had attained results of this character, his work would be much more significant than it is. But I do not think it would be easy to supply concrete instances of outstanding success in Marschak's direction by others. Marschak is describing a goal that may be attained some day, not one that has already been reached. I feel a need for more emphasis on the words "properly estimated" than Marschak has put. An investigator bent on devising demand and supply functions faces delicate issues in selecting the factors to be included in each equation, in judging what variables are so nearly homogeneous that they can be taken in aggregative form, in deciding what other variables are so heterogeneous that proper estimating is doomed to failure unless they are decomposed, and in allowing for leads or lags. I feel that the work done by Mitchell and his collaborators might prove fairly helpful in wrestling with questions such as these, and I hope that Marschak will not overlook this possibility.

However that may be, I do not see that Mitchell's failure to work out demand and supply functions signifies a negative attitude toward economic theory, or that his concrete findings concerning prices and production are of dubious value. Theoretical writers on business cycles have given a great deal of attention to the cyclical behavior of prices and production, but they have rarely worked out empirical demand or supply curves. Legislators have also been deeply concerned about the cyclical vicissitudes of prices and production, as the history of agricultural policy in this country amply illustrates. So have business managers of firms large and small. Mitchell merely follows suit in his section on prices and production, trying to establish as best he can what actual behavior has been. I do not think Marschak has conveyed adequately either the character of

Mitchell's results or his wish to probe further. Let the reader judge for himself after pondering Mitchell's concluding remarks on prices and production:

> In these few paragraphs we have flushed a rather terrifying list of theoretical problems. Why should most commodity prices rise when supply is being enlarged and fall when supply is being reduced? Why should the reference-cycle amplitudes of prices be greater than the corresponding amplitudes of production when producers cannot adjust output to current demand, and why should prices fluctuate less than production when producers can control output? Why should the cyclical movements of prices be more uniform than the cyclical movements of production, and yet have on the whole lower indexes of conformity to business cycles?
>
> Price theory may be the most highly developed section of economics, but it has not been designed to raise or to settle problems of this character. Quite obviously these problems involve the relations of present prices to past prices of the same goods; the relations of present prices of different goods to prospective profits and family comfort, and the relations of all prices to the supply of 'money' – the most ambiguous of economic terms, and therefore perfectly adapted to use in a list of unknowns. Not until we enter upon our full analysis of what happens from stage to stage of a business cycle will it be prudent to attack these complications (pp. 174-5).

Regrettably, Mitchell did not reach this stage. If he had, his explanations might still have proved inadequate. But can there be any doubt about his recognition of the importance of going beyond empirical generalizations?

In fairness to Marschak I should note that he qualifies his appraisal. Thus he writes at one point: "Mitchell saw the need for using our knowledge of human behavior and institutions." Yet in fairness to Mitchell I must quote Marschak's very next sentence: "But one may disagree whether he used it, or intended to use it, in the most efficient way." Poor Mitchell! Would he ever have claimed, any more than would any other true scientist, that he used any of his limited knowledge in the most efficient way? On the other hand, was he so wicked that he did not even try? I do not feel a need to pursue these questions; for I think I have said enough to suggest that Marschak's and Mitchell's attitudes toward economic theory are not so far apart as Marschak believes. True, economic theory did not speak to Wesley Mitchell with the clear and confident voice that it apparently uses when speaking to Marschak, but it did speak volubly to him and he to it. If Marschak had speculated less about Mitchell's *attitude toward* and concentrated more on his *contribution to* "knowledge of, or hypotheses on, human behavior", I believe his evaluation of Mitchell's work, apart from being more just, would have been more constructive.

4) Nevertheless, what I like best in Marschak's paper are some of his broad pronouncements on methodology. I am heartened by his plea that

economists combine knowledge of time series with such insights as they have into human behavior, and that they take serious account of changing institutions and techniques in their reasonings. I am encouraged by his declaration that "economic laws, far from being inexorable, depend on habits, institutions, and policies". May these views by an outstanding econometrician be heeded widely, especially by the mathematically inclined who so often speak glibly of "stable" functions without bothering to specify the boundaries of time and space to which the functions relate!

I think it is proper to add, however, that Marschak is echoing Mitchell, not urging a different position. No economist of our generation fought harder or more consistently than Mitchell for the view that "economic laws, far from being inexorable, depend on habits, institutions, and policies". Mitchell did not acquire the reputation of being one of America's leading "institutional" economists by slighting the role of human psychology, or of changing institutions and techniques. Should anyone doubt this, he will make a good start toward revising his opinion by merely reviewing the titles of Mitchell's numerous publications.

I may recall also that Mitchell's long and active career was not confined to pure science. He gave considerable time, both in official capacities and privately, to the subject of economic planning. He labored as he did on the puzzle of business cycles because he wished to help his fellow men to overcome the agony and wastage of business depressions. I know of nothing in Mitchell's life or work that would support Marschak's opinion that Mitchell regarded the business cycle as immutable, as a phenomenon man could do little or nothing about. To be sure, Mitchell worked with the concept of a typical cycle, but there is nothing very peculiar or exceptional in this. Most business-cycle theorists utilize such a concept, implicitly if not explicitly. To infer from its use a belief in an immutable cycle makes as little sense as to infer from the use of the equilibrium concept a belief in a stationary economy. If something other than Mitchell's effort to develop the typical characteristics of business cycles has led Marschak to his strange opinion, I cannot imagine what it might be.

5) Marschak has told an interesting story about beetles, but the moral I draw from it is a little different from Marschak's. He invites the reader to watch from a skyscraper "a procession of beetles, moving, stopping, moving, stopping". By using a stopwatch and recording the observations, one would obtain a good cyclical pattern of stops and moves. By observing the colors of the beetles, one might find in them a cyclical pattern also. All this, Marschak observes, is sound empirical knowledge, but surely the reader would be equipped with more useful knowledge if he realized that the beetles are really cars that stop and move according to traffic signals. This story brings out effectively how our powers of prediction and control

may be increased when we learn the "mechanism" that lies back of empirical regularities. Apparently, Marschak also thinks there may be some resemblance between Mitchell and the man counting beetles and their irrelevant colors, and that Mitchell's work is therefore not likely to be very fruitful. But, as far as this is concerned, Marschak's story begs the main issue – namely, how to discover an underlying mechanism when one does not yet know it. Let the reader note carefully that while the counter of "beetles" is generating nonsensical "facts", he is not necessarily free from "theory". On the contrary, he may be strongly under the spell of some theorist's vision that a relationship exists between the color of beetles and their rate of motion. Let the reader note, too, that while Marschak unravels skilfully the mystery of the "beetles", he gives not the slightest hint of the secret surrounding the cyclical behavior of Mitchell's time series. If Marschak's notions concerning business cycles approximated his knowledge of the mechanism of traffic controls, he would hardly be silent at this juncture. That, I think, is the most that can be charged against Mitchell.

A scientist searching for an explanation of a complex phenomenon cannot very well avoid making some false moves, despite the insights and hypotheses he has accumulated by wrestling long and patiently with his problem. I have no doubt that Mitchell made many. But a scientist does not take lightly empirical regularities because they are empirical, any more than he takes lightly a causal hypothesis because it is as yet untested. Rather he seeks to go beyond the one and beyond the other; for his aim is not something loosely called "theory" or "empiricism", but to reach generalizations that are fully dependable and have wide implications. In science as in ordinary life dependability is often a matter of degree, and one gets along with what one has until something better turns up. In science as in ordinary life the implications of a general proposition, whether it be an empirical regularity or one fortified by reason, are rarely, if ever, all seen at once. Empirical regularities, if firmly established, are precious, although sound theoretical knowledge is more precious still. That, I think, is the true moral of Marschak's parable of the beetles.